Differentiating and Quantifying Anthropogenic Organic Matter from Natural Organic Matter in Urban Runoff Collected at NEMDC

M. Lee Davisson

Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0310: Differentiating and Quantifying Anthropogenic Organic Matter from Natural Organic Matter in Urban Runoff Collected at NEMDC

Final Panel Rating
adequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

The project background is based on the observation that organic matter is very important to the management of the bay–delta system because of its dual role in ecosystems (nutrients and energy for higher trophic levels) and as contaminants (precursor to disinfection byproducts and within the organic pool are specific man–made contaminants). The goal of the project is to better differentiate between anthropogenic and natural organic matter in sources, processes, and fates. The project research area is the greater Sacramento urban area; this is an area where major anthropogenic inputs occur and also the Sacramento River and Delta region is a major source area of drinking water. It is estimated that about 10% of the total organic carbon (TOC) load of the Sacramento River is from this area, funneled through the Natomas East Main Drainage Canal (NEMDC). This study area is projected with major growth of human population in the near future and thus the differentiation between TOC of anthropogenic and natural origins is critical. Recent estimates suggest that autochthonous primary production has declined as allochthonous anthropogenic TOC inputs have increased. There is need to better quantify the current TOC picture and to be able to project impacts with future population growth. The project will assess aerosol and

particulate matter that gets into the water (this is a TOC source usually not considered in most aquatic studies) as well as storm water runoff. Bioavailability studies will be made of the storm runoff and to chemically fractionate the TOC pool for a better understanding of sources. Attempts will be made to quantify total TOC of anthropogenic origin contributing to storm runoff from the urban area (including aerosols and particles, impervious surface runoff, and vegetation and soil). It is difficult to categorize the complex components of the TOC; but some level of characterization is needed. The proposed research plans to do some separations and quantification of components. While the PIs have some expertise here, they appear to have limited experience with extensive characterizations of natural and anthropogenic TOC pools. Davisson has considerable experience with isotopic analysis and use of tracers to understand dynamics of organic matter. He proposes to use this expertise and provides an interesting and unusual perspective with the two−member mixing model differentiation of ancient carbon (due to solution of aerosols and particulates from fossil fuel combustion products) and modern carbon from photosynthesis. This is certainly a strong element in the proposed project. The lack of a hypothesis−based research plan and the apparent more limited experience with some of the isolation and separation methods for specific organic classes weakens the overall proposal.

Additional Comments:

The project background is based on the observation that organic matter is very important to the management of the bay−delta system because of its dual role in ecosystems (nutrients and energy for higher trophic levels) and as contaminants (precursor to disinfection byproducts and within the organic pool are specific man−made contaminants). The goal of the project is to better differentiate between anthropogenic and natural organic matter in sources, processes, and fates. The project research area is the greater Sacramento urban area; this is an area where major anthropogenic inputs occur and also the Sacramento River and

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Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

The proposal seeks to address the contribution of anthropogenic TOC and other contaminants by an urban flood control project (located North of Sacramento). The two external reviewers agreed that the proposal was “very good”; however, both reviewers appeared to have identified significant technical gaps in the program; thus, their ratings are not consistent with their comments. Technical panelists considered this to be an important topic; however, they worried that the applicants lacked sufficient background and expertise in certain parts of the project to ensure that these components would be completed successfully. For example, the proposal to study bio−availability using radio−carbon techniques is not well−designed. Also, it does not appear that the PIs have sufficient experience with the organic chemical analyses proposed.

Rating: Adequate

Technical Review #1

proposal title: Differentiating and Quantifying Anthropogenic Organic Matter from Natural Organic Matter in Urban Runoff Collected at NEMDC

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**The goals in the proposal are very innovative and address a very important question in light of the expected population growth in the study area. The goals also seem to be realistic and based on extensive background information which the authors presented clearly in their proposal. The proposal includes a number of new and creative aspects and sounds promising for very interesting findings. Some of the analyses techniques listed in the sample processing section are not mentioned in the section describing the goals of the proposal.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**The background information has been compiled nicely for this proposal and the author seems to be aware of the existing knowledge and preliminary results. The intellectual basis of all the planed sampling methods, sample treatment, and analyses is laid out clearly and the proposers seem to be confident with all the procedures. There are some challenges in this proposed**

	study but I think the potential outcome merits tho take those risks in order to obtain new information that probably forms an important base for future studies. The study could yield an easier way to predict the quantity and effects of anthropogenic DOM for the Delta system.
Rating	**very good**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**The authors explain in detail the planned approach of the study indicating they put a considerable effort into planning this proposal. The methods suggested in the proposal are state of the art and built on very important work done by internationally well established scientists like Jerry Leenheer. Results of this study, due to their complexity will definitely add new and important information to our knowledge base. The value for decision makers will be baseline information to predict organic matter input in the future under higher population pressure. Some of the objectives in the proposal are very challenging and seem to be oversimplified. I am especially skeptical that a two endmember model for 14C in organic matter will adequately represent the real world, however, I also feel it is worth a try and see what the data say in the end. I am also a little concerned about the amount of work involved with sample preparation and the very complex analyses. I wonder if enough personnel is involved to successfully complete the task.**

Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The approach is documented in enough detail and it seems that the proposers have a grasp on the theoretical background of all the sampling and analytical procedures suggested to be used in this study.**
Rating	**very good**

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	**The study is mainly observational/experimental and not a classical monitoring program. However, it could be a very important pilot study for later monitoring activities of anthropogenic carbon inputs into the Delta system.**
Rating	**not applicable**

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	**Products are not specifically addressed in the proposal but the study would definitely contribute new knowledge and fill important gaps to understand the NEMDC system, an important region of freshwater sources for 20 million people. The rapidly rising**

	population in the region also represents an important reason to understand natural versus anthropogenic processes effecting drinking water quantity and quality.
Rating	**good**

Additional Comments

Comments	**none**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**As indicated before the work load seems high for 3 junior investigators. The proposers have experience with some analytical procedures but not with many of the more detailed chemical analyses. There is mention of cooperation but the budget does not indicate the involvement of subcontractors. One might also be concerned that the primary staff in the project do not have a Ph.D. potentially indicating lack of experience with complex projects as this one. A lot of the proposed analyses will need to be performed outside of the proposers laboratories. At this point the proposal does not adequately describe who and how all the analytical work will be performed.**
Rating	**good**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget is very lean and seems reasonable for a two year project if no outsourcing of analyses**

	is required.
Rating	**very good**

Overall

Provide a brief explanation of your summary rating.

Comments	**I really enjoyed reading the proposal and hope it will lead to interesting new information. The proposers did a thorough job compiling the information and base their research on state of the art techniques. The combination of the various organic matter fractions and numerous characterization methods are very promising. At the same time they require a lot of work and expertise for a rather small team of not so experienced researchers.**
Rating	**very good**

Technical Review #2

proposal title: Differentiating and Quantifying Anthropogenic Organic Matter from Natural Organic Matter in Urban Runoff Collected at NEMDC

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	**This is an important problem. The understanding of sources of organic matter in aquatic systems in general lags far behind that of other materials, in part due to the complexity of possible mixing. The PIs propose a method by which these different sources can be deconvolved, with a particular focus on differentiating major anthropogenic and natural sources of organic matter. Goals and objectives are clear, hypotheses are not stated.**
Rating	**excellent**

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

Comments	**Existing knowledge of potential organic matter sources to the study site is adequately reviewed, and demonstrated to be lacking. The PIs have a clear model of the main sources, and how they will identify them based on earlier studies of different regions. The proposed scale of the project is justified in my view, if successful it could streamline monitoring programs.**

Rating	**excellent**

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	**Overall, I find the approach forward-thinking and appropriate to the objectives of the study. Based on cited publications, all the proposed work can be carried out, and results will undoubtedly advance the base on knowledge. there are a few specific aspects of the approach that I believe the PIs should take into account. 1. One possible source of "radiocarbon dead" organic matter that they do not discuss is the weathering of organic rich sedimentary rocks, such as black shales, in the watershed. I don't know about their specific watershed, but this has been demonstrated as significant in other California streams (see work by Neal Blair). 2. Sample sizes of runoff may be too small. My work has been in lake and marine systems, but we had to filter on the order of 1000 liters to obtain sufficient organic material for detailed biomarker analysis. It is possible that the waters to be sampled are much more organic rich than those in my experience, but the PIs should at least consider that possibility.**
Rating	**very good**

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	**The proposed methodology is quite new, however, it has been published (or is at least in**

	press), suggesting that it is a feasible plan. Based on descriptions in the proposal and background of the PIs, i would rate the liklihood of success high. The authors certainly have the capability of carrying out a project of this scale.
Rating	very good

Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	Adequate methodological blanks will be run.
Rating	excellent

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	A major contribution from the proposed research would be added knowledge of the sources of anthropogenic and natural organic matter to the study site. In addition, the methodology proposed would likely lead to improvements in monitoring strategies. For example, rather than looking for a large number of specific molecules, they are looking at broad compound classes, and using radiocarbon to further separate them. Thus, based on the results of this study future monitoring efforts will likely be able to focus only on the most relevant compound groups.
Rating	excellent

Additional Comments

Comments	**This is solid sciencs that I feel should be funded. As far as I can tell, it is well within the bounds outlined by the funding authority.**

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	**The research team is qualified, and has an excellent track record of production. Infrastructure etc. is more than adequate.**
Rating	**excellent**

Budget

Is the budget reasonable and adequate for the work proposed?

Comments	**The budget is reasonable for the work proposed.**
Rating	**excellent**

Overall

Provide a brief explanation of your summary rating.

Comments	**I think this is a very solid, well thought out proposal. the only reason that I did not give it an excellent rating is that I feel some aspects of the methodology required a bit more thought. I do believe that this proposal should be funded, and that the PIs will take comments into account when implementing their project.**
Rating	**very good**